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Author(s): Nancy Cartwright

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The Metaphysics of the Disunified World¹

Nancy Cartwright

London School of Economics

Pluralism is usually opposed to realism. That's why realists tend to affirm reductionism, even if only the lapsed reductionism of supervenience. It is no accident that postmoderns talk about the different worlds we live in. The realist is bent upon one world with one history, and that is the history for the sciences to tell about it, albeit with different degrees of precision, for different purposes and different points of view. The opposition between realism and pluralism is multiplied when the domains of different theories float about as in the balloon image of the relation of the sciences (Figure 1)¹ and when no combination of fields can together supply a set of descriptions in terms of which at least one baseline history can be told. But the opposition is not necessary. A devotion to realism, to the faith that there is one history to be told and differences in the telling come only from a stress on different aspects, need not turn one from pluralism. The contrary view arises, I think, from too narrow a conception of the metaphysical alternatives. What I want to do here is to broaden that conception, to propose a plan for how to build the metaphysics of the disunified world.

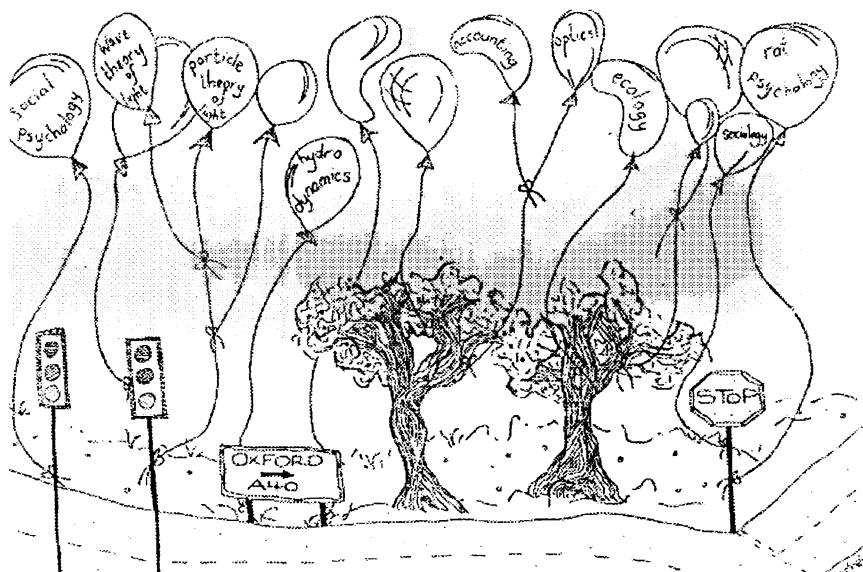


Figure 1. The Balloon Image of the Relation of the Sciences.

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My assigned subject for this symposium is physics, so I shall confine my considerations to the joint issues of realism and pluralism in physics. I begin with a claim that I have long defended: the laws of physics are true only in models. Realists very reasonably do not like this. The very precise fit between theoretical prediction and experimental results that occurs in the best cases argues too strongly against it. Social constructionism is often turned to the antirealist cause here: This very precise fit is almost universally confined to the laboratory; the theories we fashion are good only for the objects we make. Nevertheless by any ordinary standards it seems right to say that the theory is true of those objects whose behaviour it so precisely describes². But this can be accommodated and in a way that incorporates the social constructionist challenge. "Theory is true only in models" is shorthand: The theory is true only in those situations that resemble its models. Laboratory experiments and the objects of scientific technology are chief among these, and for good reasons. We build our devices to fit our well-understood models, for then we will know what to expect of the devices.

I can illustrate with the simplest most familiar case of Newton's law, $\mathbf{F}=\mathbf{ma}$. There is a tendency to read this as a universal truth of all objects of a specified kind. Any object with inertial mass \mathbf{m} will undergo an acceleration equal to $1/\mathbf{m}$ times the force exerted on it. I think we have learned no such thing, nor do our successes with applying mechanics argue for it. To predict what will happen in a mechanical system we must piece together a good (or good enough) description of it from our stock of standard models. These models are crucial to the content of the theory. Hempel and Nagel saw them as crucial too, but for them it was a matter of meaning. "Force" is a theoretical term only partially interpreted by its role in a system of laws. The rules that tell us how to assign force-functions in standard models serve to provide it additional meaning by connecting it with terms that are antecedently understood. Questions of meaning and content aside, the point remains: "Force" is connected with real systems only via a set of models that assign force-functions to specific kinds of situations. Let us look at some of these standard models (Figure 2—5):³ Simple harmonic motion, damped harmonic motion, elliptical motion, motion in a uniform magnetic field. The first line of the figure in each case gives the abstract "theoretical" description: say, motion under a force $-\mathbf{k}\mathbf{x}$. But when is a motion "under a force of size $-\mathbf{k}\mathbf{x}$ "? That is the point of the model. We have pictured here one standard case, and there are clearly a number of others. As Kuhn pointed out, learning the family resemblances that make all the cases " $-\mathbf{k}\mathbf{x}$ " cases is a good part of what learning physics amounts to. I claim that what physics teaches is just the kind of fact pictured in these figures: When you have a situation that (sufficiently) resembles this, you get a motion like that; in situations like this, a motion like that; and so forth.

But isn't that just what the semantic view of theories says? Theories just are collections of models. I think not. One for a trivial reason. This view tends to obscure the condi-

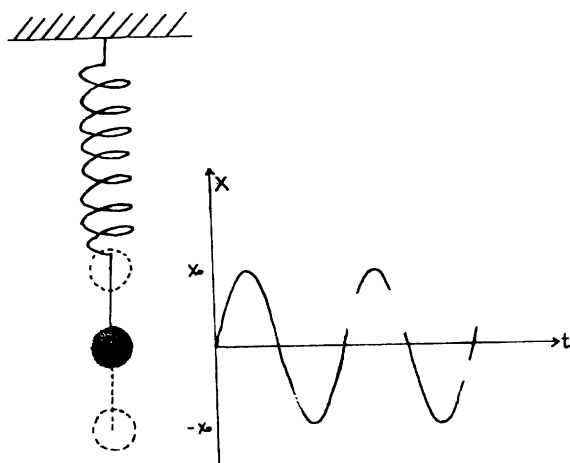


Figure 2. Simple Harmonic Motion: The motion of a body under the influence of a restoring force proportional to the displacement $F(x) = -kx$

tional claim relevant for both testing and application. It is not enough for theory adequacy that there be situations that “resemble” this with motions like that. It must be true that if a situation resembles this then the motion will be like that. The second reason is more to the point today. Think how the set of models that constitute a theory is to be characterized. Not, we know, as models of a set of axioms in some formal language. Nevertheless the models are usually thought to be sets of fictional objects characterized in terms of theoretical properties appearing in the laws—in our case \mathbf{F} , \mathbf{m} , \mathbf{a} —exhibiting, or approximately exhibiting, the relations prescribed in the law: \mathbf{F} equals \mathbf{m} times \mathbf{a} . The models of Figure 2—5 are characterized in a different way—they are springs, or pendula or dipole oscillators—i.e. oscillating charge distributions in an atom in an external field. Ronald Giere, like me, wants to focus on these kinds of models; not ones assigned abstract properties like \mathbf{F} but rather ones that provide concrete functional forms (like $-\mathbf{k}\mathbf{x}$) for the abstract term “force”. But I don’t think he draws what appears to me as an immediate conclusion: If that is what the theory is, the theory is very limited in its domain. Any situation that does not resemble a model of the theory will not be governed by its laws.

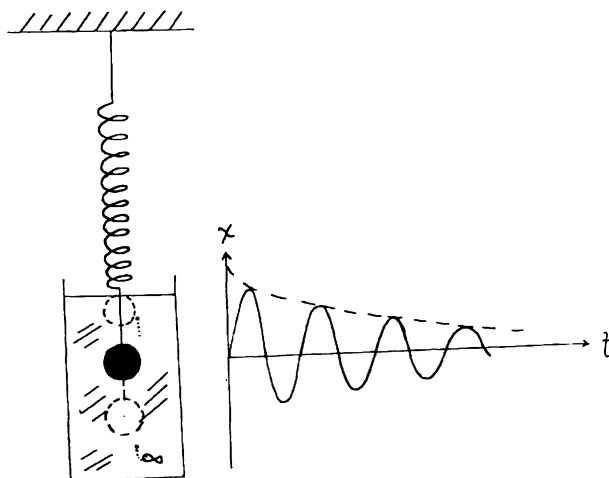


Figure 3. Damped Harmonic Motion: A simple harmonic motion with an additional force directly proportional to the velocity $F(x) = -kx - bu$

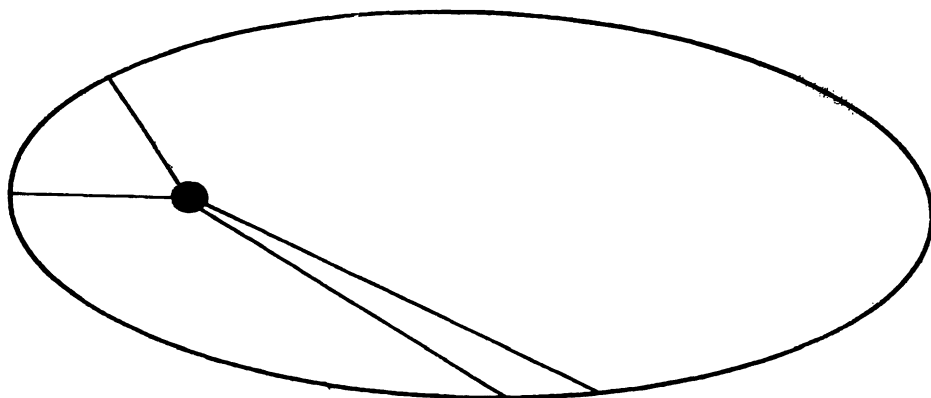


Figure 4. Elliptical motion: The motion of a body under the influence of a central force inversely proportional to the square of the distance

Consider for example a falling object. Not a nice compact one like a pound coin, but something more vulnerable to non-gravitational influence. Otto Neurath has a nice example. My doctrine about the case is much like his.

In some cases a physicist is a worse prophet than a (behaviourist psychologist), as when he is supposed to specify where in St Stephen's Square a thousand dollar bill swept away by the wind will land, whereas a (behaviourist) can specify the result of a conditioning experiment rather accurately (Neurath 1987).

Mechanics provides no model for this situation. We have only a partial model that describes the thousand dollar bill as an unsupported object in the vicinity of the earth and thereby introduces the force exerted on it due to gravity. Is that the total force? Those who believe in the unlimited dominion of mechanics will say no. There is in principle (in God's completed theory?) a model in mechanics for the action of the wind, albeit probably a very complicated one that we may never succeed in constructing. This belief is essential for the universal applicability of mechanics. If there is no model for the thousand dollar bill in mechanics, then what happens to the note is not determined by its laws. Some falling objects, indeed a very great number, will be outside the domain of mechanics or only partially affected by it.

What then fixes the motion of the bill if mechanics is not enough? I suppose it is too disturbing to suggest nothing. The effect of the action of the wind follows no systematic pattern. But we do not need to maintain that no laws obtain where mechanics runs out. Fluid dynamics may have loose overlaps and intertwinings with mechanics. But it is in no way a subdiscipline of basic physics; it is a discipline on its own. Its laws can direct the thousand dollar bill in addition to those of Newton.

Here begins the promised reconciliation of realism and pluralism. Fluid dynamics can be both genuinely different from and genuinely irreducible to Newtonian mechanics. Yet both can be true at once because—to put it crudely—both are true only in systems sufficiently like their models, and their models are very different. Mechanics studies hard objects, compact or rigid; fluids are floppy, extended, permeable. They do not easily fit any of the standard models that fix the extension of “force”.

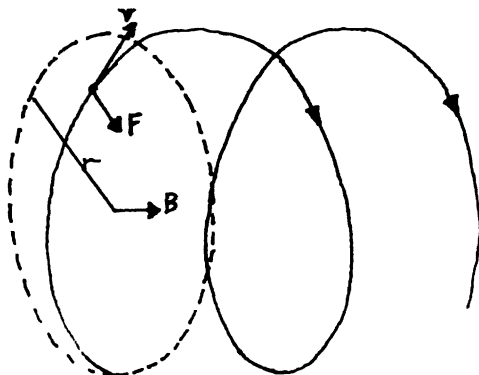


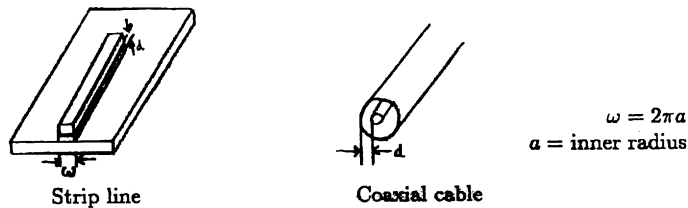
Figure 5. The motion of a charged particle of velocity v in a uniform magnetic field. The force exerted on the particle is $F = qv \times B$.

But if we are to maintain that different theories can each be true in its own domain and yet in no way reducible to an experiment on each other, what prevents inconsistencies when objects fall in the domain of both? For a general discussion, besides Dupre (1993) is Suppes (1984). I recommend Humphreys (1994) and Mitchell (1992).

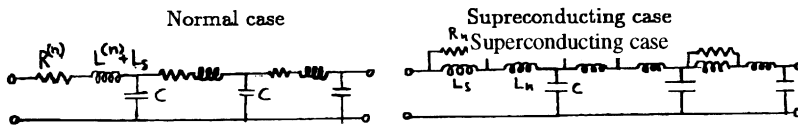
Specifically about the case of the thousand dollar bill I want to argue that Newton's law $F=ma$ is, like all laws a *ceteris paribus* law. It tells us how the acceleration of an object changes so long as nothing interferes, where “interferes” has a quite specific interpretation. $F=ma$ is true so long as no influences on the acceleration occur that can not be modelled as a force. I have written about this in somewhat more detail in Cartwright (1994).

Here I would like in the last section to look more generally at the case of two theories and how they overlap—quantum mechanics and classical mechanics. This is a good case

Superconducting strip line and coaxial cables.



Corresponding lumped parameter equivalent circuit.



$$\delta^{-2} = \mu_0 \sigma^n \omega \text{ (skin depth)}$$

$$\lambda^{-2} = m / \mu_0 n e^2 \text{ (penetration depth)}$$

with characteristic parameters

$$L(\text{per unit length}) = u_0 / \omega (d + 2\delta)$$

$$L(\text{per unit length}) = u_0 / \omega (d + 2\lambda)$$

$$C(\text{per unit length}) = e\omega / d$$

$$C(\text{per unit length}) = e\omega / d$$

$$R^n(\text{per unit length}) = S / \delta \omega = 1 / \sigma^n \delta \omega$$

$$R^n(\text{per unit length}) = 2(\lambda / \delta)^3 R^n$$

$$v = \text{phase velocity} = \sqrt{1/LC} = \bar{c} \sqrt{\frac{d}{d + 2\delta}}$$

$$v = \bar{c} \sqrt{\frac{d}{d + 2\delta}}$$

$$\bar{c} = \sqrt{1/u_0 \epsilon}$$

$$z = \sqrt{L/C} = z_0 \sqrt{d(d + 2\delta) / \omega^2}$$

$$z = z_0 \sqrt{d(d + 2\delta) / \omega^2}$$

Figure 6

because it is generally assumed that we have discovered that quantum mechanics is true of some cases we had hoped to treat using classical mechanics and hence classical mechanics is false. I think just the contrary. All evidence points to the conclusion that I really like—that Nature is not reductive and single-minded. She has a rich, and diverse, tolerant imagination and is happily running both classical and quantum mechanics side-by-side⁴.

At the end of a measurement we are told usually that the apparatus and system are in a composite quantum state that is a superposition across eigenstates of the apparatus pointer observable. But the pointer, we know, points in a definite direction. It has, to macroscopic accuracies, a definite position. So how do we get from here to a problem? Basically by assuming that all true descriptions are renderable as quantum descriptions. The pointer has a position. We used to have a classical physics that treated positions: Systems with position were assigned classical states and the behaviour of these states was encoded in classical mechanics. If we are going henceforth to use only quantum mechanics all these descriptions must go, and we will have to find some analogue in quantum mechanics for the pointer position that is so well treated by classical mechanics. The best candidate seems to

be an eigenstate of an operator we dub “the pointer observable”. But this quantum state is incompatible with the Schrödinger-evolved state. Hence the measurement problem.

My proposed strategy, consistent with the kind of theoretical pluralism I have been advocating here, is not to succumb to the quantum takeover. The world is rich in properties—they are all equal citizens. We long ago learned that there are properties like positions and momenta which are well represented by classical mechanics. The discovery that there are also features that are well represented by quantum states and well treated by quantum theory does not in itself give us reason to throw out those properties that have been long established. So my claim is this: There are both quantum and classical states and the same system can have both without contradiction. It is important here that I say classical states, not quantum analogues of classical states. There is no contradiction built in because we have no theory (nor even a good programme for such a theory) of the relation between quantum and classical characteristics. As with all cases of genuine theoretical pluralism, what we have to do is look for what connections there are and where they are. The job we have to undertake is not that of solving but rather of hunting the quantum measurement problem.

I say there is no theory of the relation between quantum and classical states. What then of the generalised Born interpretation?

Born Interpretation:

Every linear Hermitian operator $\hat{A} = \sum e_i |\Phi_i\rangle \langle \Phi_i|$ corresponds to an observable A, such that for systems in quantum state ψ :

$$\text{Prob}(A = e_i) = |\langle \psi | \Phi_i \rangle|^2$$

I don't understand this so-called interpretation. We use in it the mysterious expression “observable” but what we find represented by quantum operators is far from observable in any reasonable sense of the term. It might mean properties that a quantum system may possess, but if so it misses out a lot. Some quantum systems are for instance coloured. Nor can we fall back on what for most theories would be the right sense—these operators represent “the causally efficacious” properties or those “governed by law”—since in quantum mechanics this privilege falls to the quantum state. Worse, it does not in the end get us what we need for, as it is usually understood, systems which exhibit one of these allowed values e_i are not in nice classical states that behave in nice classical ways. They are rather still in quantum states (here Φ_i), and to keep their behaviour in line with classical predictions where we know them to be accurate, they have to spend all their time surreptitiously being measured and decohering. I propose we give up on this formula. We neither use it nor need it.

I turn first to the claim that we do not use it. How then do we relate quantum and classical properties? It seems nature has no general formula, or at least we haven't found it. The association is “piecemeal” and proceeds in thousands of different ways in thousands of different problems. Figuring out these connections is a good deal of what physics is about, though we often fail to notice this fact in our fascination with the abstract mathematical structure of quantum theory and quantum field theory. Consider superconductivity. This is a quantum phenomenon. We really do need quantum mechanics to understand it. Yet superconducting devices are firmly embedded in classical circuits studied by classical electronics. This is one of the things that most puzzled me in the lab studying SQUID's. You'd wire the device up, put it in the fridge to get it down below critical temperature, and then turn on the switch. Very often you simply wouldn't get the characteristic I-V curve which is the first test that the device is operating as a SQUID. What has gone wrong? To figure it out the experimenter would begin to draw classical circuit diagrams. Without going all the way to SQUID's, we can see in figure 6 an example for the simplest kind of superconducting configuration that can be found in any standard treatment of superconducting electronics.

“What allows you to draw classical circuit diagrams for these quantum devices”, I would ask. The reply: “There are well known theorems that show that any complicat-

ed circuit is equivalent to certain simple circuits". But that missed my point. What allows us to associate classical circuits with quantum devices?

No matter what theory you use—London, Ginsberg Landau (Gorkov), BCS—all have as a central assumption the association of a familiar quantum quantity

$$\mathbf{J}_s = (\mathbf{e}^* \mathbf{h} / \mathbf{m}^* \mathbf{i}) (\psi^* \nabla \psi - \psi \nabla \psi^*) - (\mathbf{e}^* / \mathbf{m}^*) |\psi|^2 \mathbf{A}$$

with a classical current that flows around the circuit. I say it is familiar because this is just what, in the Born interpretation would be described as a probability current, taking $|\psi|^2$ as a probability and using the conventional replacement for situations where magnetic fields play a role

$$\nabla \rightarrow \nabla \pm (ie^* / \hbar) \mathbf{A}.$$

Yet we have all learned that we mustn't interpret $e |\psi|^2$ as a charge density as Schrödinger wished to do. One of the reasons is that it cannot usually be expressed in the co-ordinates of physical space but needs rather some higher dimensional co-ordinate space. But in this case it can be. And we have learned from the success of the theory that this way of calculating the electrical current is a good one.

Does this not give us back the Born interpretation? No. On the Born interpretation what we have here is a probability and a probability in need of an elaborate story to prove that: (i) provides a mechanism that keeps reducing the paired electrons of the superconductor so they have a position and (ii) in some way or other ensures that the mean value (or something like it that evolves properly enough in time) is the one that almost always occurs. We have no such story and we need no such story. This formula is not an instance of a general probabilistic interpretation of quantum mechanics but rather an empirically well-confirmed context-local rule for how a quantum state ψ is associated with a classical current in a superconductor. So here we see that even in a case that looks very much like an application of the Born interpretation, it is not really the Born Interpretation that we are using.

My second point is not just that we don't use the Born interpretation, but also that we don't need it. How then do we interpret quantum mechanics? Notice first that the discussion of the measurement problem usually presupposes a strongly realist view about the quantum state function. People like me who are prepared to use different incompatible state assignments in models treating different aspects of one and the same system are hardly troubled by the contradictions that are supposed to arise in special contexts of measurement. But it is puzzling why quantum realists should be calling for interpretation. For those who take the quantum state function seriously as providing a true and physically significant description, the quantum state should need no interpretation. There is no reason to suppose that those features of reality that are responsible for determining the behaviour of its microstructure must be tied neatly to our "antecedent" concepts or to what we can tell by looking. Of course a fundamental property or state must be tied causally to anything it explains. But laying out those ties need look nothing like an interpretation.

What I want to stress here is that quantum realists should take the quantum state seriously as a genuine feature of reality and not treat it as an instrumentalist would, as a convenient way of summarising information about other kinds of properties. Nor should they insist that other descriptions cannot be assigned besides quantum descriptions. For that is to suppose not only that the theory is true but that it provides a complete description of everything of interest in reality. And that is not realism, but imperialism.

Conclusion: First the sermon. Resist the quantum takeover. All evidence is that quantum states and classical states can live peacefully in the world together. Indeed resist all takeovers. Second is the metaphysics: You don't have to be a social constructionist or a relativist to resist takeovers. Once you are willing to take seriously that for the most part there are no universal formulas for how the features studied by different disciplines relate, even a realist can live in a mottled dappled world.

Notes

¹This paper is part of the “Research Project in Modelling in Physics and Economics” at the Centre for the Philosophy of Natural and Social Sciences, London School of Economics and Political Science.

²By Rachel Hacking; taken from N. Cartwright, J. Cat, K. Fleck, T. Uebel, Otto Neurath: *Philosophy between science and Politics* Cambridge University Press

³For most cases this is not my own view since I do not believe the theory does describe what happens. It serves rather as one tool among many to produce a model. The model describes what happens, more or less well, and it is not a model of the theory in the logician’s sense of “model” nor even approximately like one.

⁴By G. Zouros, Department of Philosophy, Logic and Scientific Method, London School of Economics.

⁵The point of view towards quantum mechanics described here is argued in more detail in Cartwright (forthcoming) “Where in the World is the Quantum Measurement Problem”.

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